caster and Clearport; Pickaway county—Tarlton; Ross county—Adelphi, Hallsville and throughout Green, Union, Concord and Buckskin townships; Highland county—in Paint and Northern Marshall townships; Adams county—the vicinity of Winchester; Kentucky—in the vicinity of Carthage, Burlington and Woolpers creek.

Figure 10 shows more distinctly the relation of the glacial limit to the Ohio river at Cincinnati, producing the supposed ice dam discussed in a previous number of the NATURALIST (see Vol. XVIII, June, 1884, pp. 563–567), and the line across Indiana traced by me last summer. The northern part of Dearborn, the whole of Ripley, Decatur, Jennings and Bartholomew counties are deeply covered with true glacial drift, and the extreme limit is pretty easily ascertained, though the deposits in Jefferson, Clark and Scott counties are scanty as compared with the counties farther north. The highest point of the State is in Brown county, 1150 feet above the sea. The ice deposits do not reach to that point, but are very deep and extensive a few miles north over the southern part of Johnson county. In Owen county there are numerous striæ running 50° east of south, or nearly at right angles to the glacial limit. The glacial deposits in Southwestern Indiana are covered with "loess," which is doubtless a water deposit, and will, to the westward, probably greatly increase the difficulty of tracing the exact southern boundary of the glaciated area.

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ON THE EVIDENCE THAT THE EARTH'S INTERIOR IS SOLID.

BY DR. M. E. WADSWORTH.

(Continued from page 686, July number.)

Conclusions.—Starting with the common belief that the earth was once an intensely hot gaseous body, it follows that when cooled from a gaseous to a liquid state, convection would cause the intermingling of all the liquid portions only so long as the heat kept every part at the same density. As soon as an especial difference in density manifested itself (if it had not already done so in the gaseous state) the heavier materials would sink towards the interior and the lighter pass outward towards the exterior. So soon as these materials became viscous the inter-

change would be retarded. Now, when convection no longer caused the heterogeneous materials of the earth to mingle, the cooling rate would change from the comparatively rapid rate of convection-cooling to the very slow rate of cooling by the conduction of liquids.

It is to be remembered that to have convection in liquids at all there must be some external source which shall, at some point, continually supply an increment of heat, but for a cooling globe no such supply exists. These are facts that ought to be taken into account in all discussions relating to the age of the earth or sun.

It would seem, however, that Thomson's view of the age of the earth is based upon the supposition that the earth during its liquid state was homogeneous and cooled throughout by convection, and that later it became solid and likewise cooled by the ordinary conduction of a solid body.

The writer would hold, in contradistinction, that after the earliest stages the liquid earth cooled by conduction in a heterogeneous liquid, and after the superficial crust was formed, by conduction not only through a heterogeneous liquid, but also a heterogeneous and, at least in its exterior portion, a more or less discontinuous or fragmental solid. In this way it would seem as if biologists might gain a portion, if not all, the time desired, which is now denied them by the physicist.

In the same way, if the heavier gases tend to lie nearest the center in a hot gaseous body, the exceedingly slow rate of cooling on account of the poor conductivity of gases ought to be taken into account in all discussions relating to the age of any body formerly gaseous. Another factor would be the heat disengaged by the chemical unions necessary to form the mineral combinations, now existant on the earth, out of the once disassociated gases.

But to return; when the lighter surface material of the earth had cooled sufficiently, a crust would be formed which, owing either to its lighter state in its hot condition, or to its scoriaceous character and the viscidity of the material beneath, would not sink. It is to be remembered that on account of the passage of rocks through the softened or viscous state to the solid, that the viscous material immediately below the solid crust would be in nearly the same condition and temperature as the overlying crust

into which it would gradually pass. It is not probable that the crust would break up and begin to sink, because even if its surface grew cold it would always have this hot, solid base, lighter than the underlying viscous liquid, which, owing to the increasing specific gravity as the interior was approached, would probably be more dense than any of the overlying cold crust.

Even if the crust should become heavier, break up and begin to sink, this sinking would be very slow on account of the viscosity of the liquid and its constantly increasing density, while the heat imparted to the sinking crust would tend to bring it to about the same specific gravity as the liquid, as the sinking mass neared its melting point. But above and beyond all, it would soon reach a point at which the liquid, being of different composition, had a higher specific gravity than the crust, and no farther sinking could take place. We should thus expect to have formed on the earth's surface a crust which never would sink, or if it sank at all, would for only a comparatively short distance, giving rise at that point to a solid crust floating upon a denser heterogeneous liquid. While willing to admit that the crust when cold would be heavier than the liquid out of which it was formed, it is denied that the exterior would cool to such an extent as to be heavier until solidification had taken place to sufficient depth to render the contraction of the exterior portion of but little effect; that is the increased density of the liquid immediately beneath the hot lighter interior portion of the crust would more than counterbalance the increased density of the cold exterior portion of that crust.

Sir William Thomson's idea of a crust on solidification sinking to the center of the earth and building up a honey-combed mass, is only applicable to a homogeneous liquid globe of but slight viscosity, whose material contracts in passing from the liquid to the solid state.

In such a condition of the earth as the writer supposes, a gradual passage from the cooled surface crust towards the hotter interior portions of that crust, thence into the plastic and viscous condition, no opportunity would exist for the generally supposed shrinking away of the nucleus from the rigid crust, but the entire earth would contract as a whole, causing a linear shortening of the crust through compression. This would occasion a crushing together of this crust, causing it to be depressed in some

places and elevated in others. The depression of any portion of the crust into the viscous liquid beneath would cause the elevation of an equivalent weight of the liquid material; as in the case of ice, the depression of the ice on one side causes the heavier water to overflow unless it can escape in some other direction. The simple sinking of a portion of the crust on one side with its corresponding but less elevation on the other, with the attendant fissuring, affords all the dynamic agent needed to raise lavas to the top of the highest mountains ;[1] while if in any way the yielding to the lateral compression should be sudden, instead of gradual, owing to fracturing and slipping of the parts, an earthquake shock would result.

If the general views of the compression of the material in the interior of the earth are correct, then if from any cause the pressure were removed, the natural expansion of the material, if liquid, would cause it to rise to some extent in any vent or opening.

During the earlier times when the crust was thinner and the internal heat stronger, a greater variety and amount of materials raised as lavas through the fissures would be expected, and not improbably outflows of two different kinds might take place at the same time, as it would seem had taken place on Lake Superior.

The up thrust of the still liquid and yielding interior portions through the fissures in the overlying crust, and the subsequent solidification of the intruded material, would cause that crust to be tied through and through with the underlying mass.

Neither is it to be expected that the contraction would be equal in every portion, while the depression of the crust into the interior would give rise to unequal thicknesses of that crust, to which the liquid outflows would add. The great irregularity of the under surface of the crust, coupled with the gradual passage from the solid to the viscous liquid interior would conspire to prevent any of the supposed slipping of the crust over the interior, as many physicists have assumed would take place if the earth had a liquid interior.

If it is held that volcanic rocks are derived from the re-liquefaction of the original crust of the earth, would not the best theory be, in the light of what is now known of the behavior of

[1] Whitney, "Earthquakes, Volcanoes and Mountain-building," p. 90.

rocks on their solidification, that increased pressure, brought about by contraction of the crust in cooling or by sediments deposited on a sinking area, or by some other cause, produce a lowering of the fusing point, as in the case of ice, and thus enable the natural heat of the rocks themselves to cause their passage into the liquid state ?

It has been claimed with apparent justice that the simple depression of any portion of the earth's crust into the still liquefied portion of its interior, would tend to cause the base of the depressed portion to re-liquefy through the greater heat to which it would be then subjected to, thus making the re-fusion the natural result of the earth's contraction.

It appears to the writer that so far as any evidence now exists regarding the earth's interior, it is allowable to assume its present liquid state. A state that in his judgment accords better with the facts of petrography than any other assumption that has been made.

It is true that if the materials of the earth's interior were solid, but could be liquefied by diminution or increase of pressure, this liquefaction would perhaps be consonant with what is now known of the internal structure of rocks, especially the partial dissolving of the olivine of basalts, the hornblende of the andesites, the quartz of the rhyolites, etc. One of the greatest difficulties in the way of this supposition is to understand why the same lava should produce different crystals when it was in the interior from those yielded on the exterior of the earth.

It is difficult to see how, if the earth is solid, that any relief from pressure could take place otherwise than from the crushing together of the overlying rocks, the tearing up of these from the underlying ones, and elevating them into the air; that is, the relief from pressure would come from an elevating instead of a sinking process. In truth it would seem that eruptions and mountain building or elevations arose rather from the sinking of large masses causing smaller ones adjacent to rise, or, as announced by Dana, the highest border is on the side of the greatest ocean.[1] It would seem that elevation followed subsidence, instead of subsidence following elevation. If this is the case, it is difficult to explain how subsidence could be brought about first in a *solid* globe.

[1] "Man. Geol.," 1880, p. 28.

We cannot imagine that matter so rigid as the earth's interior is claimed to be, could yield to the pressure of sediments, glaciers or lava flows, as has been advocated. This view is based chiefly on the fact that areas of thick detrital formations must have been areas of subsidence, hence, it is argued, the deposit itself has been the cause of the sinking. The reverse appears rather to be true, that only areas of extended subsidence can be areas of great deposition. May it not then be claimed that the subsidence was the cause of the deposition instead of the deposition being the cause of the subsidence; and is not the former view more natural than the latter?

The deposition of sediment in any locality requires that one portion of the earth's crust should be lower than another. In the theory of a solid globe this would be brought about by the elevation of a portion of the crust, while in the theory of a liquid globe by the depression of a portion of that crust.

In a viscous mass, such as the earth's interior next the crust is here supposed to be, coupled with the irregular thickness of the crust, no especial connection could be expected to exist between different vents, even if near one another, until after the lapse of considerable time — the viscidity itself preventing any rapid motion of the interior mass.

Whatever water was met, on the welling up to the surface of the lava, would naturally render the latter more liquid, so far as it entered into the lava. The intervention of water in a volcanic eruption seems to be mainly its action on the lava during its passage upwards, instead of its being the cause of the eruption. It, indeed, plays a striking rôle in volcanic phenomena, but it does not seem to be the *primum mobile*. It is difficult to see how lava in ascending to the earth's surface could reach it without meeting water somewhere on its way. When the water was met could the results be different from those now witnessed? Does it not seem that water is the accident rather than the cause of the eruption, and do not most observers transform an effect into a cause?

It may be said that the physical evidence advanced in behalf of its essential solidity is violated by the premises and limitations chosen as the basis of the mathematical discussion; while the petrographical and geological facts demand either an interior that is liquid or one that can readily become so.

It may indeed be said with Professor Dana: "Among geological facts none appears to demand for its explanation a rigid globe. The demand has come through the supposed requirements of physical laws, studied with the aid of the highest mathematics, whose methods and conclusions are sure only when all the modifying conditions of the problem are thoroughly understood.

"It is now admitted by some of the best of physicists that no arguments have yet been presented which prove the earth to be a rigid globe, or to have a rigid crust a thousand miles or so thick; and it is also admitted by some mathematicians and physicists of eminence, including Airy, the astronomer royal, that the hypothesis of a thin crust over a liquid interior is probably the true one.

"The science of geology is, therefore, free to adopt the conclusion which seems best to suit known facts."[1]

For further discussions of the state of the earth's interior the reader is referred to

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[1] "Man. Geol.," 1880, p. 812; see also Whitney's "Earthquakes, Volcanoes and Mountain-building," 1871, p. 74.